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DISCUSSION AND CORRESPONDENCE

THE METHODS OF ENGLISH ETHNOLOGISTS

IN a letter with regard to Dr W. H. R. Rivers' "declaration of independence from the traditional point of view of his compatriots," viz. "the idea of evolution founded on a psychology common to mankind as a whole," Dr R. H. Lowie (*Science*, xxxiv, 1911, p. 604 f.) concludes: "The significant fact remains that one of the most distinguished of English ethnologists now finds himself in substantial agreement with the position generally held in America." Dr Lowie seems to imply that the attitude in America is in common agreement as to the multiplex cultural, with correspondingly various evolutionary, development, and that Rivers is the only English ethnologist who has come to this conclusion.

Dr Lowie refers to more than one American writer who has recognized this question of "dispersive evolution" and multiplex cultural development. He has, however, made no reference to what the present writer finds one of the most crucial discussions of the whole question of comparative ethnology, to wit, Dewey's article on "The Interpretation of Savage Mind," which appeared in *The Psychological Review* ten years ago (vol. 9, 1902, pp. 217-230).¹ Dewey says:

"Comparison as currently employed is defective—even perverse—in at least three respects:

"(1) It is used indiscriminately and arbitrarily. Facts are torn loose from their context in social and natural environment and heaped miscellaneous together, because they have impressed the observer as alike in some respect. Upon a single page of Spencer (*Sociology*, I, 57) . . . appear Kamschadeles, Kirghiz, Bedouins, East Africans, Bechuanas, Damaras, Hottentots, Malays, Papuans, Fijians, Andamanese—all cited in reference to establishing a certain common property of primitive minds. [Query: Is he silent about "THE GOLDEN BOUGH" because its pages offer lists too long for insertion as illustration?] What would we think of a biologist who appealed successively to some external characteristic of, say, snake, butterfly, elephant, oyster, robin in support of a statement? And yet the people mentioned present widely remote cultural resources, varied environments and distinctive institutions. What is the scientific value of a proposition thus arrived at?

¹ When the above was written Professor Boas' review of Graebner's *Methode der Ethnologie* (*Science*, Dec. 8, 1911) had not appeared.

"(2) This haphazard uncontrollable selection yields only static facts—facts which lack the dynamic quality necessary to a genetic consideration.

"(3) The results thus reached, even passing them as correct, yield only loose aggregates of unrelated traits—not a coherent scheme of mind."

I agree with Dr Lowie that "questions of priority or misunderstanding are relatively unimportant"; at the same time, just as he feels called upon to correct an apparent misapprehension on the part of Dr Rivers as to the methods of American workers, so the present writer would like to assure him that were he (Dr Lowie) "telescopically gifted, he would assuredly read nothing but amazement and surprise" in the eyes of some, at least, of the English anthropologists, "as they peruse his extraordinary [implied] characterization of their activity." For Royce and the American ethnologists are not the only ones who have recognized the principle that "not Quantity, but Order, is the fundamental category of exact thought about facts"; and it was an Englishman who wrote: "For no definite precept can be more than an illustration, though its truth were resplendent like the sun, and it was announced from heaven by the voice of God. And life is so intricate and changing, that perhaps not twenty times, or perhaps not twice in the ages, shall we find that nice consent of circumstances to which alone it can apply." "It is," says Tylor, "of as little use to be a good reasoner when there are no facts to reason upon, as it is to be a good bricklayer when there are no bricks to build with." (*Early History of Mankind*, p. 56.)

As a student for three years in one of the larger English universities, I have had impressed upon me the principle of caution and critical attitude in dealing with anthropological data, as well as the necessity of finding comparative culture that is comparable before making inductions—and this with a thoroughness and emphasis to which I have found no comparison whatsoever as a student in America. By way of introduction to a thesis presented at Oxford nearly two years ago, I said in part, as regards method:

"Our material must be gathered from the same setting, so far as the facts may be supposed to be related, the one to the other. The method by which it should be collected must be at least two-fold: first, *intensive*; second, *comparative*. By *intensive*, we mean that no result is of any value unless you have carefully and, so far as possible, exhaustively, treated the particular case with which you are engaged. It will not be sufficient to say that you have found such and such correspondences and such and such differences. This has little worth unless you go further and ascertain how far these may be held to be the total of correspondences and the total of differences; and, perhaps more important still, to what extent these similarities are more than mere chance correspondences and represent

really efficient factors. Now, it may be that these factors are not equally efficient; accordingly, it is incumbent upon us to discover the supremely efficient ones and to assign them their due place in a hierarchy of values. To be *intensive*, then, means, merely to be thorough in our study of each individual case that we take up. The importance of this thoroughness cannot be too much insisted upon, since anything short of it, if it does not actually vitiate results, at least gives them no claim to our respect. A first glance and superficial consideration of certain relations may point obviously in one direction; while a more thorough understanding of the case, involving a study of closely related conditions, may lead us to quite other conclusions. A most difficult thing to decide, for example, is whether the given correspondences are merely matter of fact ones or are intimately related.

"An intensive study is a necessary preliminary to the understanding of any particular case. But the conclusions deduced from a study of any given case may not be applicable to any other than itself, indeed may be contradicted by the conclusions of some other case. Hence, in order to adduce any principle or law or tendency, we must make a study of many cases—that is, we must study comparatively the individual cases which have been studied intensively. We must synthesise the results of these various analyses.

"In our comparative study we must bear in mind this important consideration, viz. that comparisons must be of like individuals or of like groups: there must be a common background, a common setting for the trait or tendency whose relational aspect we study, since these are colored and conditioned by all sorts of external and internal circumstances and 'cribbed, cabined and confined' by social influence and local prejudice. We must know something of these extraneous as well as of these inner influences before we can make true comparisons.

"In no aspect of the problem has the comparative method been more abused than in its anthropological aspect. Students have serenely ignored the important truth that facts about the life and beliefs of a people can receive no interpretation aside from their setting. A vast quantity of similar concretes ruthlessly abstracted from their proper setting proves nothing by this multiplicity beyond that same resemblance by virtue of which they were brought together. Anthropological treatment has almost invariably suffered from the results of such inane methods and the monotony should be broken by more rational ones. . . . And, most of all, should these facts be counterbalanced and offset by any inference drawn from whatever masses of material and give to all evidence its due weight. For that reason, in every comparative study, it seems wise and safest to study with utmost care a few tribes rather than glean at large from many fields."¹

¹ My entire anthropological training had been English, and both my examiners (the Reader in Psychology at Oxford, and the Reader in Ethnology at Cambridge) were English!

This thesis, an attempt to study the conditions of the development of personality among savages, was submitted to the anthropological department of one of the universities in this country, kept by them some eight months, and finally returned to

Dewey's words may have resounded to good effect through the corridors of Chicago and of Columbia, but a sleeping, dogmatic formalism still lurks in some nooks and crannies of other American institutions known to the writer, where methodology limps lamely in the ruts of a century ago. Indeed, in view of the fact that in more than one of the larger American universities, courses based on Spencer's *Descriptive Sociology* still flourish, and facile deductions of universal import out-Spencering Spencer himself still evolve, it is time to look to ourselves as well as to look abroad.

That some, at least, of the English anthropologists share the "American" view as to multiplex origins—a view that seems to me no more American than English—I know from my close acquaintance with their methods and the manner of presenting their subjects to classes. Indeed, were I to speak from my own experience, I should be forced to say—what, of course, is true only in my own experience—that ready inference from insufficient premises is characteristically American, while caution, broad and critical survey, and absolute lack of any tendency toward dogmatic assertions best flourish on English soil. Balfour, who has devoted some thirty years to comparative technology, was ever and anon pointing out the different lines of evolution in different areas and the caution with which we must regard any tendency as being general, or applicable to an area where it has not actually been found.

If I should forget all else about Marett, Tylor's successor as Reader in Social Anthropology, I could never forget that he was forever cautioning us against the liability of taking apparent for real similarities, and the necessity of making comparisons, if at all, from culturally similar regions. His criticisms of the Frazerian method and his departure from Frazerian interpretations and inductions seem to have received little attention in America and a word in that regard may not be out of place here. His views seem to me of such intrinsic value, aside from their bearing upon the immediate topic of discussion, and so little appreciated on this side of the water that I beg leave to make use of the following extracts which will present his point of view more correctly and convincingly than any comment of mine.

me without a word of comment. Either it was a worthless subject, a wrong method, or an impossible solution. To criticize either one's method or one's result is helpful and cause for gratitude; to ignore one's interest and effort altogether is, to say the least, tantalizing. But perhaps as Mr Findlay says, "life would not be tolerably agreeable if it were not for its amusements."

With regard to his use of the term "pre-animistic," for example, Marett says (Preface to *The Threshold of Religion*):

"What I would not be prepared to lay down dogmatically or even provisionally is merely that there was a pre-animistic *era* in the history of religion, when animism was not, and nevertheless religion of a kind existed. For all I know, some sort of animism in Tylor's sense of the word was a primary condition of the most primitive religion of mankind. But I believe that there were other conditions no less primary; moreover, I hold that it can be shown conclusively that, in some cases, animistic interpretations have been imposed on what previously bore a non-animistic sense. But, with regard to the use to which the word 'pre-animistic' has been put by such writers as Dr Preuss, Dr Farnell, Mr Clodd, Mr Warde Fowler, Mr Hodson, and others, I take it . . . that 'non-animistic' would have served most of their purposes almost as well."

I am told that a renowned head of one of the large departments of one of our largest universities tells his classes the following: "In all primitive religions there are these four elements: Magic, Tabu, Animism, and Totemism." Such was the up-to-date nineteen-eleven pronouncement. Compare with this the following from Marett's article on "Religion (Primitive)" in volume 23 of the *Encyclopædia Britannica*, where he says (p. 63):

"This fundamental homogeneity of primitive culture, however, must not be made the excuse for a treatment at the hands of psychology or sociology that dispenses with the study of details and trusts to an *a priori* method. By all means let universal characterization be attempted—we are about to attempt one here, though well aware of the difficulty in the present state of our knowledge—but they must at least model themselves on the composite photograph rather than the impressionist sketch. An enormous mass of material, mostly quite in the raw, awaits reduction to order on the part of anthropological theorists, as yet a small and unsupported body of enthusiasts. Under these circumstances it would be premature to expect agreement as to results. In regard to method, however, there is little difference of opinion. Thus, whereas the popular writer abounds in wide generalizations on the subject of primitive humanity, the expert has hitherto for the most part deliberately restricted himself to departmental investigations. . . . The anthropological expert . . . insists on making the primitive point of view itself the be-all and end-all of his investigations. The inwardness of savage religion—the meaning it has for those who practice it—constitutes its essence and meaning likewise for him, who after all is a man and a brother, not one who stands really outside."

A decade ago (see *Classical Review*, xv, 1901) this writer was remarking upon the copious pigeon-holes into which theorists were shooting the raw facts of anthropology more or less at random—simply because

they *had* to fit somewhere. Little wonder that Westermarck's array of facts and authorities gave the Oxford don somewhat the sensation of an avalanche of books about to fall on one's head. "Not quantity but quality" is the one strain that runs through all his criticisms and discussions, with an undertone of "method! method! method!" For example, in a review of the second volume of Westermarck's *Origin and Development of the Moral Ideas*, we read (*Athenæum*, April 3, 1909):

"To lack of control on the part of the live memory is chiefly due that special fallacy of the compiler, *ignoratio contextus*. Facts are unwillingly divorced from their literary context, or, more fatal still, from their sociological context. Even Prof. Westermarck, we believe, who strives so hard to be critical in his use of his authorities, has occasionally included spurious, or at any rate wrongly labelled, specimens in his vast museum of anthropological materials. . . . Granted that the civilized man, considered in abstraction from his social inheritance—in a word, from his education—is the natural man or worse, does it follow that, when the results of the socializing process have been counted in, there will remain any fundamental similarity in respect to ethics as tried by the test of conduct? It was Darwin himself who said that the difference between savage and civilized man is the difference between a wild and a tame animal. Had he laid greater emphasis on the part played in the moral life by social tradition, Prof. Westermarck might, we think, have done more to bring this difference out."

In his review of Frazer's *Totemism and Exogamy* (*Athenæum*, June 11 and 18, 1910), Marett gives a similar caution:

"The time has come to work by regions. This is not to abandon the hope of discovering universal tendencies amid the bewildering variety of man's efforts after culture. It is simply to defer that hope until we are in a better position to appreciate each piece of evidence in relation to that organic context whence most of its significance is derived. . . . Some day . . . it is to be hoped, the rising generation of anthropologists, following the rule *divide et impera*, will produce a series of sociological monographs capable of serving as the basis of the sounder comparative studies of the future. . . . Totemism, by these two enthusiastic apostles of the world-wide method has been brought so near home that the fact that British dukes habitually abstain from eating strawberry leaves would, doubtless, sooner or later have been set down to a sacred tradition cherished in secret by families whose sense of ancestry is exceptionally strong.

"It is to be hoped in the interests of such relatively final justice as science must try to dispense, that, as anthropologists increase in numbers and intensive cultivation of their not unlimited field becomes a virtue born of necessity, this duty of filtering to which Dr Frazer did not feel equal (having several continents waiting for him after he finished with Australia) will be performed thoroughly and conscientiously, as befits researchers who are aware that gold may be found in grains as well as in nuggets. . . . We may be allowed to wonder—speculatively,

as it were, and without prejudice—whether the digest of information concerning this or that region touches attainable finality. Thus the totemism of a peculiarly interesting region, the North-West Coast [of America] is, as perhaps no where else, intimately bound up with the whole social system; so that it might be argued that no study short of a thoroughgoing sociological investigation of the institutions of this group of tribes could make the meaning of their totemic practices and beliefs intelligible in all their aspects."

"Now," as Marett well knows, "there are those champions of a sociological method who have accused British anthropologists in general of substituting unconsciously for that *formule d'ensemble* of the historian, which should correspond to some gradual development actually observable in the world of fact, a pseudo-historic and merely logical representation of how something corresponding to a concept, or even to a mere name, sprang spontaneously into being; which mode of origination, they allege, is regular in ætiological myth, but rare in nature." By what right the writer of the above should share that reputation I do not know, beyond the indisputable fact of his geographical position—weighed against which his comparative isolation as an original and critical thinker seems of no moment. Does the following sound Frazerian? (See *The Birth of Humility*, Oxford, 1910, p. 6.)

"No isolated fragment of custom or belief can be worth much for the purposes of Comparative Science. In order to be understood, it must first be viewed in the light of the whole culture, the whole corporate soul-life, of the particular ethnic group concerned. Hence the new way is to emphasize concrete differences, whereas the old way was to amass resemblances heedlessly abstracted from their social context. Which way is the better is a question that wellnigh answers itself."

If these extracts do not suffice to convince the reader of what he may look upon as the un-English attitude of the Oxford Reader in Social Anthropology, let him peruse still one more—or better yet, let him read the whole of the article entitled, "The Present State of Anthropology," which appeared in the *Athenæum* (Mar. 12, 1910), and from which the following is quoted. Nor am I aware of the writings of any American author in which these points have been better presented or in any respect more significantly stated. Marett says:

"On the part of the select few who have tried to master the subject thoroughly, there is an evident desire to introduce exacter methods at all costs. There is no need to seek far for the cause of this anti-popular movement. It lies in the disillusionment which is the inevitable sequence of that gay and irresponsible time during which a youthful science sows its wild oats. Once let it be realized

that it is equally necessary to take account of similarities and of differences when employing the comparative method, and a clean sweep has to be made of the greater part of the standard anthropological authorities, with their naïve scheme of a worldwide unilinear evolution. Intensive study is the demand of the new era. It is seen that, before profitable comparison can be instituted, the things compared must be severally known to the bottom. Each datum must be constructed in the light of what Dr Farnell has termed 'the adjacent anthropology,' namely, the whole context of culture to which it is organically related. Thus the rising generation of experts is content to forego entirely the delight of reconstituting, by means of the imaginative manipulation of snippets, the age of magic, the totemic stage of society, the epoch of the undivided commune, and similar wholesale phases of 'the great might-have-been.' Its interests are local and specific. A definite anthropological province such as Australia, or even a smaller area such as the North-West Coast of America, is nowadays held to provide scope enough for the energies of the most ardent 'comparativist'. In this way, it is hoped, there will in time be given to the world a number of departmental digests serving the double purpose of furnishing the theorist with well-tested material, and indicating to the field-worker what gaps in the evidence he should endeavour to fill. . . . They [Spencer and Gillen] have shown to what unplumbed depths the most unpromising types of savage culture do in fact reach down. Henceforth, to arrive, in Mr Dennett's telling phrase, at 'the back of the black man's mind,' must appear as a most formidable undertaking—one that calls as it were for a fully equipped anthropological 'Challenger.' To haul in a few bucketfuls of surface water has scientific values no longer. Once established then, this demand for deep-sea dredging is bound to create a supply."

By way of conclusion, I may say that in my experience the Englishman's ignorance of American methods is no more colossal and no more a matter of deprecation than our ignorance of *his* methods and theories. Upon my first appearance here I was very voluminously assured of the narrowness and time-honored insularity of the English, particularly as scientists. Perhaps they are narrow, but it has occurred to me—and the impression persists—that if Americans are broader, they are correspondingly thinner in content and more superficial. This, however, is merely exemplifying the very fault which I hoped to point out: our too ready characterization of a nation or a class and the ready generalizations from insufficient data, usually as false as they are facile. And, lest the purport of the above be misunderstood, let me say that though Dr Lowie's letter to *Science* has called this forth, I have no reason to believe that he is one of those who have made the false and facile deductions that we must all deplore.

In closing, I may be allowed to refer to Marett's *Anthropology*, a review of which will be found in *Current Anthropological Literature* of

January–March, 1912. Finally, as to national scientific methods I may be allowed to quote the following from the pages of a letter written me by an English ethnologist (though I do it without having asked permission), which I feel must express the sentiments of all concerned. In regard to this article and the question of ‘American’ and ‘English’ methods, he wrote:

“I think there is something in what Lowie says, only, of course, he exaggerates. Don’t you exaggerate either! Let us keep mere partisanship out of anthropology, and rather take the wiser point of view, from which one sees that discussion and the clash of postulates all make for truth in the long run: and remember that Science is cosmopolitan—not English *or* American, but both and neither. The one thing to transcend is parochialism—which is not any the better, if it is a matter of hemispheres and continents.”

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NOTES ON CERTAIN USAGES RELATING TO LINGUISTIC WORK

INTELLIGENT variations and innovations in use of terminology, alphabetic symbols, and punctuation should be permitted and encouraged by editors and publishers and all who have charge of the form in which linguistic material is presented. It is only by variation in usage that progress in these branches has been made in the past. Rigid uniformity in these matters has, in our present state of knowledge, nothing to commend it; it means death to progress. One quickly and easily becomes familiar with any terms or symbols used by any writer, provided that their value is explained, that they are employed because of scholarly reasoning, and that they are sanely conservative. It matters not at all if one finds *s*, *ſ*, *c*, or *u* employed in various records of various languages for the sound of English *sh*. The fittest of these characters will eventually survive. Any one of them should be allowed to stand unchanged, but not unchallenged, in published linguistic work. One should be allowed to have the abbreviation for doctor printed either Dr. or Dr., according to preference. At the same time much attention and discussion should be given to such usages.

The relative merits of the practices discussed in the following brief notes ought to be thought out and tried out. We should then hold to those which are best.

1. It appears that most languages distinguish between what we may term roots and formal elements. If we call roots when compounded